The Impact of Welfare Waivers on Female Headship Decisions

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Abstract

While much of the focus of recent welfare reforms has been on moving recipients from welfare to work, many reforms were also directed at affecting decisions about living arrangements, pregnancy, marriage and cohabitation. This paper focuses on women's decisions to become or remain unmarried mothers, that is, female heads of families. We assess the impact of welfare reform waivers on those decisions while controlling for confounding local economic and social contextual conditions. We pool the 1990, 1992, and 1993 panels of the Survey of Income and Program Participation (SIPP) which span the calendar time when many states began adopting welfare waivers. For its descriptors of local labor market conditions, the project uses skill specific measures of wages and employment opportunities for counties. We estimate models for levels of female headship and proportional hazard models for entry and exit from female headship. In the hazards, we employ stratified Cox partial likelihood methods and investigate the use of state fixed effects or state stratified hazard models to control for unmeasured state influences. Based on data through 1995, we find limited evidence that work-encouraging waivers had a beneficial effect by reducing female headship of families. We find little evidence that family caps, teenage coresidence requirements or termination limits will reduce the number of single-parent families.

The Impact of Welfare Waivers on Female Headship Decisions

I. Introduction

In the 1990s, states proposed dramatic modifications in the Aid to Families with Dependent Children (AFDC) program, the largest cash welfare program in the U.S. These welfare reforms were accomplished by states obtaining waivers of standing federal welfare policies. In 1996, the president signed the Personal Responsibility and Work Opportunity Reconciliation Act (PRWORA), a major reform bill that gives states greater authority in designing and implementing their welfare programs. Under the PRWORA, many states have continued reforms initiated under waivers. While much of the focus of these reforms has been on moving recipients from welfare to work, many reforms were also directed at affecting decisions about living arrangements, pregnancy, marriage and cohabitation that affect the well-being of children. Notably, the goals of the PRWORA explicitly include ending welfare dependency by work or marriage, reducing the incidence of non-marital fertility especially for teens, and encouraging the formation and maintenance of two parent families (Maynard et al., 1998).

Much of the assessment of welfare reform has been based on mandated federal evaluations of waiver-based state demonstrations. These evaluations are usually state specific and correspond to the particular constellation of waivers and implementation chosen by the state. The location-specific evaluations generally use experimental designs that have advantages in terms of holding constant some other confounding influences, but there are three difficulties. One, their policy and location specificity makes them difficult to generalize. Two, these evaluations provide limited information about the interaction of welfare reform with labor market and other environmental characteristics because of limited variation in the environment in a particular demonstration. Three, as noted by Maynard et al. (1998, p. 157), few evaluations focused on family structure issues. This project uses national samples from the 1990, 1992 and 1993 panels of the Survey of Income and Program Participation (SIPP) to overcome these difficulties.

Our specific aims in this paper are to model female headship decisions (unmarried motherhood) and assess the impact of welfare reform provisions on those decisions while controlling for local economic and social contextual conditions. In particular, we examine whether the provisions initiated as state waivers—and in some cases incorporated into the national legislation—deterred women from becoming and remaining unmarried mothers. We examine female headship in the context of a model in which family formation decisions depend on job options, marriage options, and past welfare use as well as welfare program rules. We develop and use local area information on labor markets to control for these influences in models of the levels of female headship and in models of individual transition rates for female headship. Before proceeding, we should mention that we do not deal directly with the PRWORA but rather with waivers adopted prior to 1996. But many key provisions of welfare reform were tested via waivers from 1992-1996 and many of these waiver provisions were continued by the states under the PRWORA. If the provisions have significant effects we should be able to detect something in the pre-1996 period.

II. Background and Significance

In recent years, policy makers have paid increased attention to the role of the welfare system in influencing family structure. The percentage of female headed families (families composed of an unmarried women with children) has risen dramatically over the past three decades from 11.5 percent of all families in 1970 to 17.8 percent by 1998 (US Bureau of the Census,1998). This trend reflects an increase in out-of-wedlock births as well as a retreat from marriage. The rise in female headship is alarming because single parenthood is associated with a host of adverse outcomes. Poverty rates and welfare dependence are much higher for single-parent families than for two-parent families (Lerman, 1996); schooling and other outcomes for children in single-parent families are also substantially worse than in two-parent families (Haveman and Wolfe, 1994; McLanahan and Sandefur, 1994). Lerman (1996) finds that over the period 1971-89, the trend away from marriage was responsible for much of the increase in child poverty.

Some analysts, such as Murray (1984), blame public assistance programs for the rise in single headship. These analysts argue that the eligibility criteria for the earlier Aid to Families with Dependent Children (AFDC) program effectively subsidized single parenthood. The provisions in the PRWORA, specifically its emphasis on reducing teenage births and encouraging two-parent families, reflect these concerns.

It is unclear from previous research, however, whether changes in the welfare program really contributed to the rise in female headship. Critics of Murray point out that the general time trends run the wrong way. Single headship continued to rise during the 1980s and early 1990s, even as welfare programs became less generous and more restrictive. Thus, if anything, changes in welfare policy since the mid-1970s should have acted like a break on single headship, not as an accelerator.

The research community has not reached consensus about whether welfare benefits greatly affect family structure. In surveying the literature, Moffitt (1992,1995,1998) concludes that welfare benefits do influence marriage, divorce, and child-bearing decisions, but the effects are not strong. Moffitt (1995) notes that there is a question about whether the lack of strong results might be due to lack of control for unmeasured cross state differences in environments including other welfare rules. In our work, we control for environments using more specific and better data methods than have previously been used. In addition, past research focused on benefit levels and not more specific welfare provisions. Welfare reform alters many rules that may affect demographic decisions beyond the effect of average state benefit levels, and we examine those rules.

Much of the literature on the impacts of welfare on family structure relies primarily on variation across states in welfare benefits to identify the effect. As pointed out by Ellwood and Bane(1985) and emphasized by Moffitt and others, welfare benefits may capture other unmeasured attributes of a state environment resulting in biased estimates of their impact. Since welfare reform has taken place at the same time as significant changes in the labor market, disentangling the impact of the PRWORA from changes in employment conditions requires careful measurement of these background characteristics. This project develops local measures of relevant employment conditions in the 1990s so that these confounding influences can be separated. The remainder of this section therefore reviews literature from two main areas: studies of female headship and its component decisions and studies of the impact of local labor markets and welfare waivers on welfare use.

An early study of female headship based on individual data is Danziger, Jakubson, Schwartz, and Smolensky (1982). They apply the simple difference in utility model originated by Becker (1981) for marriage and divorce decisions. Several other studies follow using cross-sectional data sets and conclude that AFDC benefits have a positive but moderate impact on the likelihood of female headship (Moffitt, 1990; Schultz, 1994; Winkler, 1994). Winkler (1994) finds that the significance of the AFDC effect depends on inclusion of state variables measuring attitudes such as religious fundamentalism. When state attitude variables are included, AFDC has a small effect. Moffitt (2001) uses CPS data to construct time series evidence that welfare benefits increase the incidence of female headship once we control for both male and female wages changes.

Hoynes (1997a) and Moffitt (1994) use individual person data and take the step of including state fixed effects to control for unobserved state factors. Moffitt uses CPS and finds that the positive relation between welfare benefits and female headship disappears when fixed state effects are added to the model. Hoynes (1997a) uses data from PSID on women aged 16-50. She uses a linear probability model for female headship with covariates including personal characteristics and state AFDC benefits (plus food stamps) as well as other state level political, demographic and economic variables. She finds that with the inclusion of state fixed effects or individual fixed effects, AFDC benefits do not increase female headship. AFDC benefit levels may be operating through the fixed effect, but changes in benefits do not induce changes in headship. That is, she notes that past welfare benefits may have altered the demographic composition of a state through selective migration (those with unobserved high propensity to use welfare may migrate to high benefit states). Overall, she concludes that welfare reform is unlikely to affect female headship, but she adds two caveats. First, the simple benefit variation that she examines is different from the types of rule changes made under welfare reform; thus, any extrapolation of her results must be done with caution. Second, her model is a model of "stocks" or levels of female headship, not flows into and out of headship.

Dickert-Conlin and Houser (1999) also investigate levels of female headship and find no correlation between AFDC benefits and female headship once one controls for individual effects. They find that expansion of the Earned Income Tax Credit program (EITC) affected the level of female headship, increasing headship for whites but decreasing headship for blacks. They use data from the 1990, 1991, 1992 and 1993 panels of the SIPP in a linear probability model for headship. The study uses only the observations from December of each year and thus has, at most, three observations per person. They do not use indicators of welfare waivers, which were adopted during the same time period as the EITC changes and whose effects could be confounded. In the current paper we include indicators of these waivers. We also analyze flows into and out of female headship as well as levels. This point is important because, if welfare reform has an impact, it will have a sizable effect on the flows well before it affects stocks.

The large literatures on the flow components of female headship–marriage, cohabitation, divorce, childbearing–have been surveyed elsewhere (Acs 1995, Hoynes 1997b, Moffitt 1995, Moffitt 1998). As above, they conclude that cross sectional variation in benefits produces results consistent with the hypothesis that welfare benefits affect demographic outcomes, although the effect sizes are moderate or small. Some recent evidence on the relation between marriage or non-marital childbearing and welfare is consistent with this summary (Hoffman and Duncan 1995, Hoffman and Foster 1997), yet others find larger effects (Rosenzwieg 1999). Evenhouse and Reilly (1999), Moffitt, Reville and Winker (1998) and Winkler (1995) show that welfare benefits affect cohabitation and marriage, although Dickert-Conlin (1998) finds no effect. Of course, welfare benefits are only one factor in demographic decisions. Employment prospects and wages are key determinants as well, to which we now turn.

Controlling for local economic conditions matters conceptually because wages and employment prospects affect resources available and change the effective cost of alternative demographic choices. For example, the level of wages and work choices affect the opportunity cost of having children, and the relative income from being single or married. Employment conditions must be controlled to estimate the impact of welfare program changes. Studies based on aggregate data such as Wood (1995) find that changes in employment opportunities do not explain changes in family formation for blacks and generate mixed results for whites. Matthews et al. (1997) find significant effects of women's and men's wages on fertility. Lichter et al. (1997) find that higher women's wages increase female headship whereas higher men's wages reduce female headship.

Ribar (1998) points out that a difficulty with these aggregate demographic models is inadequate measurement of personal characteristics. Ribar uses individual data from NLSY coupled with local data on employment in a joint hazard for initial fertility and marriage (that is, the first transition to each). He finds no impact of welfare benefits on fertility but does find an impact on marriage. Thus welfare contributes to more female heads by increasing the average time spent single and at risk of out-of-wedlock birth. He finds that wages do not have a consistent effect on marriage or fertility across different specifications and suggests that further investigation is necessary. Lichter et al. (2001) use individual CPS data in a model of marriage with fixed state effects and find that economic conditions matter. They find that the economic recovery of the 1990s substantially attenuated the decline in marriage.

The relation between living arrangement (coresidence) and labor markets has also generated recent interest. London (1998) uses CPS to find that the percentage of single mothers living with their parents was stable over the period 1970-95 but that there was a large increase in cohabitation. Card and Lemieux (1997) find that a deterioration in local labor market conditions increases the probability of youths living at home with their parents. Haveman and Knight (1999) find that youths, especially low-skilled youths, are shifting away from arrangements with spouse and children toward living with parents or living alone. These studies have focused on the labor market and not on welfare program impacts. The studies do point out the importance of developing good controls for labor market characteristics so as not to confound these with the impact of welfare reform.

Ribar (1999) investigates the importance of improved measurement of employment prospects and finds that low-skill women's earnings are sensitive to local employment opportunities. This study pursues these improvements.

The impact of welfare waivers has been studied in three ways. First, studies of the causes of the large reductions in aggregate welfare caseloads have tried to determine the parts that were due to welfare waiver provisions and the parts due to improving economic conditions. CEA (1997, 1999) and Blank (2001) find that employment conditions matter most but that welfare reform contributed to the caseload drop. In a more comprehensive analysis, Schoeni and Blank (1999) report that waivers do decrease caseloads. They also find that waivers may raise the probability of marriage for those with low education. Figlio and Ziliak (1999) are more skeptical of the impact of waivers. They adopt a specification with explicit dynamics and conclude that almost all of the caseload reduction was due to good employment conditions.

Horvath and Peters (1999) use aggregate state-level panel data for 1984-1996 from various sources to investigate the impact of welfare waivers on non-marital births. They stratify the analysis by age groups and control for attributes of the states including wages, unemployment rates, poverty rates and other contextual variables, sex-ratios, and policy indicators for benefit levels and waivers. They include fixed state effects and time effects. They conclude that states that adopt "any" waiver have significantly lower non-marital birth ratios. Among waivers, they find that the family cap appears to have the most consistent negative effect. Remaining component waivers offer mixed results, often insignficant or with counterintuitive effects. For example, teen coresidence provisions appear to have a positive effect on non-marital birth ratios. Time limits and work requirements appear to have little effect. The difficulty with aggregate caseload studies is that variations in the demographic composition of the caseload are imperfectly controlled and the use of state level averages may disguise underlying relations within aggregate groups.

The second type of waiver study focuses on specific provisions in specific states. For example, Grogger and Michalopolous (1999) Grogger (2001) and Swann (1998) investigate the impact of time limits on welfare use using individual data. Studies of this type and some recent studies evaluating provisions of the PRWORA are difficult to summarize because they are location specific and relate to various idiosyncratic combinations of welfare waiver provisions. Moffitt and Pavetti (1999) provide an overview of issues on time limits. For an overview of the variety of waiver programs and evaluation activity, see Harvey, Camasso and Jagannathan (2000). Greenberg et. al. (2001) review results from the multisite California GAIN and National Evaluation of Welfare-to-Work programs. They caution against relating site-to-site variation in outcomes to site-to-site in programs design, participant traits and local conditions. They demonstrate that a "macro" regression approach relating outcomes to site characteristics generally yields mixed or insignificant results. This is largely due to limited numbers of sites and small cell size for variations.

The third type of study uses individual data from national data sets and tries to gauge the impact of waivers by using the variation in waivers over time by states. Gittleman (2000) uses data from PSID and estimates models for initial entry, exit, and re-entry into welfare over the period 1983-95. He finds that welfare waivers have shortened spells in the 90s but states that this effect may be due to other changes in the environment in waiver states. This comment suggests the need for control for unmeasured state fixed effects which Gittleman did not do. Ribar (2000) uses individual data from the 1992 and 1993 panels of the SIPP and investigates how waivers and improved measures of local labor market conditions affect transition rates onto and off of welfare. He controls for both regional effects and time effects. He finds that local labor markets have large and statistically significant effects on transitions but that state welfare waivers are not statistically significant. Neither study finds evidence that waivers had large impacts, but both acknowledge that we have a relatively short post waiver period within which to observe adjustments.

In conclusion, we extend the literature in several ways, primarily by focusing on family structure and waivers. We look at welfare rules beyond average benefit levels. We model flows into and out of female headship. To estimate the impacts of welfare reform separate from changes in the economic environment, we use improved measures of local employment conditions as conditioning variables. Further, we investigate state fixed effects or state specific hazards.

III. Conceptual Framework

As noted above, there are theoretical reasons to believe that AFDC has discouraged marriage and increased out-of-wedlock births. Moffitt and Pavetti (1999) argue that time limits and other waivers that reduce the generosity of the AFDC program would be expected to increase marriage rates, decrease out-of-wedlock births and reduce cohabitation. This would increase exit rates from and reduce entry rates into female headship.

To formalize the idea, we use Becker's (1981) rational choice model of demographic decision-making. Like Hoynes (1997) and Moffitt (1994), we consider a simple two-state model in which a woman becomes or remains a single household head (i.e., becomes an unmarried mother) if the indirect utility associated with being a single household head exceeds that associated with not being a single head.

To model the female headship decision, let $u(F, W_f, 0, B_f, X)$ be the expected lifetime utility of choosing female headship today, and $u(N, W_f, W_m, B_m, X)$ be the expected lifetime utility of not choosing female headship today, where u represents an indirect utility function. F and N index female head or not, W_f is the woman's wage, W_m is the spouse's potential wage, B_f represents a vector of welfare benefits and welfare rules that determine benefits potentially available if the woman is a female head, and B_m represents a vector of welfare benefits and welfare rules if the woman is married and not a head. Most AFDC recipients are female heads. AFDC benefits are available to married couples only under the AFDC- UP program, although the Earned Income Tax Credit (EITC) is available and provides support to low income families. X represents a vector of personal characteristics and taste parameters. A woman selects female headship today if

$$F^* = u(F, W_f, 0, B_f, X) - u(N, W_f, W_m, B_m, X) > 0.$$

The arguments of the indirect utility function thus include personal characteristics, environmental characteristics, and public policy parameters. Personal characteristics would include determinants of the person's wage or potential wage, other measures of the opportunity cost and value of time (e.g., number and ages of children), and non-wage income. Characteristics of the environment would include measures of the vitality of the labor market such as local wages and employment and measures of the availability and quality of potential spouses (variations of sex ratios, male wages). Public policy parameters would include welfare benefit levels, and indicators of other relevant welfare policy (waivers for time limits, family caps, AFDC-UP, etc.).

A woman's decisions regarding marriage, childbearing and living arrangements will change over time as new information and opportunities arise. The sequence of decisions gives rise to spells for each decision: in or out of female headship. In this paper we follow others and focus on the reduced form approximation of the decision process. Structural modeling of the joint headship and welfare receipt decisions is left for future work.

The model yields ambiguous predictions about the effects of wages and employment opportunities on the probability of being an unmarried mother. On the one hand, higher wages discourage fertility (hence headship) because higher wages raise the opportunity cost of having children. On the other hand, higher wages raise income and may cause higher demand for children. As for marriage, higher wages for the woman raise utility in both the married and unmarried states and thus have an ambiguous effect. To the extent that wages in marriage will be shared, a higher wage may make the unmarried state relatively more attractive and discourage marriage. This is the "independence effect" of higher wages. As for potential employment prospects, similar arguments about the fertility and independence effects apply.

The model yields predictions about the impact of waivers on female headship. Waivers that make welfare less attractive by imposing time limits, harsher sanctions, family caps or other restrictions should discourage female headship by reducing its relative utility (reducing B_f relative to B_m). This may lead to fewer unmarried births or divorce (reduced entry into headship) or encourage women to marry (increase exit from headship). Waivers that make welfare more attractive, such as making the earned income disregard more generous, should encourage female headship. An obvious complication that is subsumed in this simple model is that waivers may affect work effort. Waivers that encourage or require work such as work requirement time limits and JOBS sanctions could reduce fertility because they raise the opportunity cost of children (child care must be arranged and purchased), but these effects may be mitigated if the waivers also subsidize childcare. Work-encouraging waivers have an ambiguous effect on marriage incentives. More work could lead to greater independence but could also alter marriage prospects, perhaps for the better.

Rules that teenage welfare recipient live with their parents or in supervised living arrangements could reduce teenage births if the goal of the teen is to establish an independent household. The availability of child care at home could encourage further schooling or work, both with complicated effects on future headship. Horvath and Peters (1999) further suggest that teenage coresidence rules could actually increase births by making the future appear more secure: the teen will either live at home or in a group setting.

Overall, even a simple model leads to ambiguous predictions regarding how waivers affect headship. The most straightforward prediction is that waivers that make welfare less desirable should discourage headship. But complications prevent us from saying much more.

IV. Data Construction

For our analysis we need individual longitudinal data on headship and welfare linked with detailed measurement of environmental and policy variables.

A. Individual data from the SIPP

The impact of PWRORA on women's decisions can best be estimated by using longitudinal data from the 1990s. We use the Survey of Income and Program Participation (SIPP), a logical data set both in content and time coverage.[1] The SIPP includes detailed information on monthly living arrangements, government program use, employment and wages, and other demographic characteristics. The panels vary in length from 32 to 40 months. We pool the 1990, 1992, and 1993 panels, which together span the interval October 1989 to December 1995. The SIPP is national survey with approximately 20,000 households per panel. The survey over-samples low-income households and some other groups; when weights (supplied with the survey) are used, estimates are nationally representative. Respondents are interviewed every four months about their monthly activities in that period. Each 4 month interview period is called a "wave." While our sample covers the period when many states adopted welfare waivers, our sample does not cover time after the adoption of the PRWORA in 1996. We plan to add data from the 1996 panel of the SIPP in the near future when an edited longitudinal file is released. Even though SIPP panels are relatively short, large panel sizes give us an adequate number of transitions. In addition, we exploit the retrospective information on marriage, fertility and migration that are gathered in SIPP topical modules. This retrospective information allows us to extend our sample back in time.

In this paper we undertake two types of analyses. We first look at levels of female headship and then at transitions into and out of female headship. On a monthly basis, we define a female head as a woman aged 15 or more who is unmarried[2] and has children in the family in that month. Female heads of subfamilies are counted as female heads. For reasons explained below, the time periods actually used in the analysis are 4-month periods called waves. The covariates in the hazard models are those taken from the fourth month of each wave. For the level of headship analysis, both the female headship indicator and the covariates come from the fourth month of each wave. The use of wave data cuts down the size of the data set. In addition, the wave data avoids the "seam" problem with monthly transition data in the SIPP whereby

[1] The PSID is soon to release the 1997 wave data, but its recent change to every other year interviews is not ideal for this project. In addition the PSID has a smaller sample size than multiple SIPP panels. The NLSY 1979 cohort will be too old and the NLSY 1997 has no pre welfare reform coverage.

[2] Women who report their status as "married spouse absent" were counted as married.

transitions are reported more frequently at months between waves than within waves. If monthly data were used, the seam problem would contribute measurement error.

For the hazard analyses, we employ two types of samples. We define spells as consecutive waves of headship or consecutive waves of non-headship. In the first type of sample, we use only the core SIPP data and measure spells that occur within the SIPP panel. As is often done, we exclude left censored spells (those that were ongoing at the beginning of the panel) and use only the complete and right censored spells that begin during the panel. This produces valid estimates of the model for the sample of new spells and will be considered our primary results. However, exclusion of left censored spells causes a considerable loss of data. Furthermore, new spell entrants may have different characteristics than those who entered their headship state in earlier periods with different labor markets and different welfare policies in place. In light of this, we develop a second type of sample for hazard analysis that exploits the retrospective information. We construct headship and non-headship spells going back from the beginning date of the SIPP panel to the woman's age 15. Constructing spells back to age 15 allows us to use spells ongoing at the beginning of the SIPP panel. Since we use all spells back to age 15, we do not have to make strong stationarity assumptions about entry rates that are often used when adding in left censored spells. But our sample has its limitations and requires its own strong assumptions, which we subsequently explain.

To provide contextual and policy information, we match in environmental variables gathered at the state and county level. The public use versions of the SIPP do not supply county of residence information, and provide MSA of residence for only a portion of the sample in order to preserve confidentiality of respondents. Even respondents in small states are grouped to protect confidentiality. By special arrangement,[3] we gained access to the internal SIPP files that include full geographic detail on state and county of residence. This allowed us to match in labor market data at the county level and to match in state data for the full SIPP sample.

B. Local Labor Markets

For its descriptors of local labor market conditions, the project exploits the recent work by Ribar (2000). Prior to Ribar's work, researchers who were interested in county-level estimates of wage and employment opportunities for low-skill workers faced an uncomfortable set of trade-offs. At the county-level, researchers who wanted year-to-year measures could use general aggregates such as the overall unemployment rate, employment in selected industries such as manufacturing or services, or total personal income, but not skill-specific aggregates as measures for economic opportunities. Alternatively, researchers could construct skill-specific aggregates from the PUMS, but only at ten-year intervals and only for counties with large populations.

Ribar constructs indirect annual measures for all counties from 1989-96 by combining skill-specific information on earnings and employment from the Sample Edited Detail File (SEDF) of the 1990 Decennial Census and the 1991-97 Annual Demographic files of the Current

[3] All data work and analysis was done at the Boston Research Data Center or the Center for Economic Studies at the US Bureau of the Census and the results approved for release.

Population Survey (CPS) with annual industry-specific information from the Regional Economic Information System (REIS). He uses special versions of the SEDF and CPS files, which identify county of residence and work [4] To construct the indirect measures, Ribar regresses the low-skill wage data from the SEDF and CPS files on a set of personal variables from the combined files and local employment and earnings measures derived the REIS. The wage regressions are corrected for selectivity from the employment decision and account for county-specific effects as well as general time effects. Estimates from the regressions are then combined with the available employment and earnings data from the REIS to impute wages for low-skill women workers across counties.

C. Welfare Program and Policy Parameters

We measure welfare benefits as the maximum AFDC benefit available to a family of three with no other income; we adjust for inflation using the CPI-U (base year 1992). Using a value for a family of three avoids the potential endogeneity of allowing benefit to depend on family size. The federal Earned Income Tax Credit (EITC) is a wage subsidy for low-income earners. It has a phase in range where wages are subsidized, a flat range, and a phase out range where the subsidy is reduced as earnings rise. Following Dickert-Conlin and Houser (1999) we used the maximum EITC benefit available to a family with two children to measure EITC generosity. Ten states have adopted their own EITC supplements so that there is some variation in EITC benefits across states at a point in time, but most of the variation is time series variation in the federal established benefits.

Many waivers of federal welfare policy were tried in states prior to 1996. Since states adopted different provisions and at different times, we get variation by state over calendar time that allows us to estimate impacts. We mostly rely on the welfare waiver indicators that were assembled by the U.S. Department of Health Human Services (DHHS) and used by the Council of Economic Advisors in their 1997 and 1999 reports. These indicators include whether a state adopted any major waiver (any), whether a state set a total time limit on benefits (Termination or Term Limit), whether a state adopted a family cap provision that limits or eliminates benefits for additional children while on welfare (Family Cap), whether a state adopted a time limit before work was mandated (Work Requirement Time Limit), whether a state applied sanctions for failure to engage in Job Opportunity and Basic Skills (JOBS) program activity or lowered the age of children for which JOBS activity was required by the mother (JOBS waivers), and whether a state adopted expanded earnings disregards (Earnings Disregard). In addition, we gathered information on whether teenage parents were required to live with their parents in order to receive welfare (Teen Coresidence Requirement).

For most of the analysis we group waivers into two indicators: an indicator for whether a state adopted a termination limit, family cap or teenage coresidence requirement (term/family waiver) , and an indicator for a Work requirement waiver, earnings disregard, or JOBS waiver (work-type waiver). These grouped waivers allow us to determine if work-type waivers have different effects from the family-oriented and termination waiver. In addition we tested the

[4] Ribar was granted access to the special files while he was a research associate at the Center for Economic Studies, US Bureau of the Census.

sensitivity of our results by using implementation dates instead of adoption dates, and by constructing indices that counted waivers.

The DHHS waiver data was compared with print sources such as the *Green Book* and electronic sources such as the Urban Institute's Welfare Rules Database. In the analysis below we use indicators that take on values of zero prior to a state adopting a waiver and one afterwards. The waiver date is the month and year that the particular provision was adopted statewide (see CEA 1997).[5] Waivers were adopted only by some counties are not counted unless and until they are adopted statewide. In specification checks we consider two alternatives. In one, we lagged the adoption dates by 9 months (12 months for the teenage coresidence since that waiver is measured yearly) to allow for a delayed response of demographic decisions (marriage and childbirth). In the other, we used the implementation date of the waiver when available as recorded by ASPE. It is not obvious which waiver date is most meaningful. Women's behavior could be affected by the announcement of policy changes (i.e. the adoption date or even publicity prior to the adoption date), or could be more affected by actual experience with the waivers (implementation dates or lagged dates). As a practical matter, our results were not very sensitive to the way that we dated the waivers and we report our primary results as those that used the adoption dates.

Table 1 shows the means of the variables used in the levels and new spell hazard analysis. Most are self-explanatory. The sample sizes are shown in person waves. The spell samples are much smaller because they include only the new spell sample, thus excluding left censored and multiple spells that would appear in the larger sample described in panel A. The proportions of states that adopted various waivers up to and including 1996 show that waiver adoption was common.

V. Results

A. Trends in Female Headship

Figure 1 shows the proportion of sample woman aged 15-55 who are unmarried mothers in each month, averaged for each calendar year. There is an upward trend in female headship in the 1990s for ages 15-55, but the trend is fairly flat after 1993. The figure also shows separate trends for those who lived in states that eventually adopted a major waiver and those that did not. The states that did not adopt a waiver show a greater rise in female headship than the non-waiver states. Most waivers were adopted after 1992, and the figure suggests that waivers had a beneficial impact—waiver states had slower growth in female headship.

This suggestion is confirmed by a simple regression analysis in Model 1 of Table 2. The dependent variable in the model is a binary indicator for whether a woman is a female head. This binary indicator is regressed on year (the time trend), an indicator for whether the woman's

[5] The teenage coresidence waiver is measured on a yearly basis based on data from the WRD. There were some inconsistencies between two waiver files (old and new) at the Urban Institute. In cases of conflict we used the new file and cases with uncertain adoption were counted as non-adopters.

state of residence ever adopted a major waiver, and an interaction between the waiver indicator and year. The negative and significant coefficient on the interaction term tells us that the trend in headship for waiver states was significantly lower than the non-waiver states. The non-waiver states show a positive trend but the trend is small (.002=.006-.004) for the waiver states. In Model 2 the ever any waiver indicator is replaced by an indicator that equals one only in years when a major waiver was in effect in the woman's state of residence. Again, the significant negative coefficient on the interaction tells us that waiver states had a significantly lower trend in headship after waiver adoption; the trend after adoption shows falling headship (-.004=.003-.007).

In models 3 and 4 we split the "any" waiver indicator into a waiver for termination/family waivers and one for work-type waivers as discussed above. The work-type waivers correlate with significantly lower trends in headship whereas the term/family waivers have moderate sized positive, but statistically insignificant effects. Thus the work-type waivers appear to generate the beneficial effects that reduce female headship. The picture and regression also make it clear that waiver and non-waiver states were quite different in female headship proportions prior to waiver adoptions. Thus we will need to control carefully for state attributes.

B. Levels of Female Headship

This simple test can be substantially improved upon by conditioning on additional variables, both the demographic characteristics of individuals, which may differ across states, and other measures of local labor market conditions and policy parameters. Table 3 shows results of a logit estimation where the dependent variable indicates whether the woman was a female head or not. We restrict our attention to women who are 15 to 55 years of age. The model uses time-varying waiver indicators that turn on when the state adopts a waiver provision. Along with demographic characteristics, we control for local labor market conditions by using predicted local log wage and employment probabilities. As mentioned above, the wage and employment probabilities are skill specific: they are assigned by education and age, as well as county and race, and vary over time. The wage provides an estimate of the potential wage for the woman, and the employment probability tells about her relative chances of finding a job. The logits also include a measure of the real value of maximum AFDC benefits and the real value of the maximum EITC benefit. All models also include year dummies to pick up unmeasured influences over time.

For waivers, we consider two specifications: the first uses the aggregate waivers for term/family type waivers and work-type waivers, while the second uses the six component waivers. These waiver variables are time varying. To control for unobserved attributes of the states we use state fixed effects in the last two columns. These fixed effects will control for unobserved time-invariant differences across states that might be due to unmeasured attributes of the states welfare system or to unmeasured differences in social attitudes towards unmarried motherhood. Both might affect a woman's choice. Thus Table 3 shows four models.

In all models, the results for the demographic characteristics are consistent with past studies. Younger women, black women, and women with low education levels are more likely to be female heads. AFDC benefits have a significant positive impact on female headship, as theory suggests, but only in the models without state fixed effects. When state fixed effects are included, AFDC benefits lose their significance. This result is consistent with Moffitt (1994) and Hoynes (1997) who suggest that benefits may be picking up other unmeasured attributes of the states in the absence of fixed state effects. The EITC is not found to have a significant effect on levels of headship (although the p-value is close to .1 in the no-fixed-effect model 2), counter to Dickert-Conlin and Houser (1999).[6]

The predicted wage rate has a positive impact on female headship, statistically significant in the models with fixed effects. As mentioned in the conceptual model section, wages have competing effects on headship. High wages would be expected to reduce fertility (hence headship) due to the opportunity cost of children, but high wages may increase headship due to the independence effect (making marriage relatively less attractive). Our estimates suggest that the independence effect dominates. The local probability of employment has a significant negative effect on female headship: women in high employment areas are less likely to be female heads. Again, there may be competing effects of marriage and fertility. Employment might be associated with lower fertility, but employment could associate positively or negatively with marriage. Further, high levels of employment for women could proxy high levels of employment for men, and more employed men could indicate a better marriage pool and hence less headship. Either the fertility effect or the marriage pool effect is consistent with our data.

The indicator for adoption of the aggregate waivers has no significant effect once we have added all the controls. Among the component waivers, two waivers have significant effects, both in the fixed effect model 4. States that adopted expanded earnings disregards have lower levels of female headship. Since expanded earnings disregards make combining work and welfare more attractive and thus raise the relative income of being on welfare, the negative sign runs counter to our conceptual model. The teen coresidence requirement has a significant positive effect on headship which is also counterintuitive. Horvath and Peters (1999) also observe a positive sign on teenage coresidence in a related context where they estimate the proportion of out-of-wedlock births by unmarried women using state panel data. They note that the result is counterintuitive because the presumption is that the waiver would discourage pregnancy by teens that become pregnant in order to become independent or avoid a bad home situation. As mentioned earlier, they argue that the odd sign might be explained because the waiver might add some security for a teenage mother who knows she will either be living at home or in a group situation.

Alternatively, the odd sign could also simply show a political endogeneity (states adopt a teenage residence requirement when they feel they have too high a level of female headship) rather than a detrimental impact of the waiver. To investigate this possibility further, we added additional contextual variables to control for the political climate. In models not shown, we added three indicators for whether the state had a majority Republican house, a majority Republican state senate, or a Republican governor. The political variables were not significant

[6]There are sample differences, methodological differences, and model differences between their study and ours. They use the 1990, 1991, 1992, and 1993 panels of the SIPP with three annual time series observations per person and a linear probability model with individual effects. They do not consider waivers. At this point, we have not sorted out the exact source of the differences. It could be methodology, or it could be that adding waiver indicators eliminates the EITC effect.

and did not qualitatively change the other coefficients. We then added a variable that might concern state legislatures: the proportion of the state population made up by never-married female heads. Again that coefficient was insignificant and the other results were robust. These findings lead us to discount the political endogeneity explanation.

Since women with low education are more likely to participate in welfare, the impact of waivers on behavior should be stronger for this group. To further check the specification, we reestimated the model with an interaction between the waiver indicators and an indicator for low education (less than 12 years). In Table 4 we show the coefficients for the waiver terms and the interactions with waivers. The models include the other conditioning variables from before, but the coefficients are suppressed in the table. For the aggregate waivers the termination/family type waivers have a significant positive effect on headship for low education women, another counterintuitive result. For the less educated women, the only significant component waiver is the JOBS sanctions. Since JOBS sanctions make welfare less attractive, hence female headship less attractive, the positive coefficient is again counterintuitive. Thus the low education interaction model casts further doubt on the idea that waivers have a beneficial impact.

In further specification checks, we included a single indicator for whether the state had adopted any type of waiver in place of the two aggregate waivers. The any waiver was not significantly different from zero in the specifications. When we used the waiver date lagged 9 months, the aggregate work and family/term waivers have insignificant effects as do the component waivers. Thus the models appear robust to these changes.

Our overall conclusion from this specification is that local wages and employment conditions are important determinants of female headship but that waivers have either little effect or counter-intuitive effects once one controls for demographic and other contextual conditions. One might argue that it will take more time for waivers to affect the "stock" or level of female headship but that we might sooner observe an impact on the flows into or out of headship. We now turn to this argument.

C. Transition Rates for New Spells from the Core Data

In this section we use only those spells that begin after the sample starts, that is, we keep only spells where we observe a woman entering female headship from non-headship or observe a woman exiting from headship. We estimate transition rates based on these complete and right censored spells that occur in the core SIPP data. We refer to this sample as the new spells sample to distinguish it from the pooled new spell and retrospective sample that we introduce later. We again restrict our attention to women who are 15 to 55 years of age. For women who are age 15 or less at the beginning of the sample, we assume that spells of non-headship start at age 15. Thus a woman who is age 15 at the sample beginning is counted as being observed to enter non-headship at the sample beginning; women who are less that fifteen are counted as beginning their spell when they turn 15. In addition, a woman could enter a spell of non-headship during the panel by being an unmarried mother who marries. For the hazard analyses, the starting and ending dates of spells are determined using the monthly data, then converted to the corresponding wave.

1. <u>Exit Rates from Female Headship</u>

A woman exits female headship in one of two ways: an unmarried woman with children marries, or all children of an unmarried woman with children move out or grow up and leave the family. In this paper we maintain a larger sample by combining these and look at exit from headship.

We specify the exit hazard as a proportional hazard model that we estimate using the Cox partial likelihood approach. This approach does not specify a parametric form for the underlying hazard but rather treats it as a nuisance function that is eliminated from the likelihood. In this proportional hazard model the covariates serve to shift the underlying hazard up or down.[7] In addition, we also estimate a model that allows for state-specific underlying hazards. This is a generalization of state fixed effects called state stratified partial likelihood estimation. While this method has been known for some time, it has been rarely used.[8] Our innovation is to apply it to controlling for state effects.

The method allows the form of duration dependence to vary freely across states, but the method constrains the effects of covariates on shifting up or down the proportional hazard to be equal across states. The method requires at least two spells in each location with at least one completed spell. States with no complete spells are ignored (that is, they cancel out of the likelihood). As it turns out, we drop data from two states because they lack of complete spells in the exit hazards and similarly drop data ten states in the entry hazards. There is a possible selection bias in that we tend to be ignoring women from smaller states, but the impact on sample size is minor. Note that these women would be dropped if we used traditional fixed effects as well. The likelihood for the stratified model is developed in the Appendix. The likelihood for the simpler non-stratified model will be apparent as the likelihood with a common underlying hazard for all states.

Table 5 shows the multivariate exit hazard. All specifications include controls for demographic characteristics, local labor market conditions, year effects, and policy parameters. Again we show four models: 1 and 3 with the aggregate term/family and work-type waivers indicators, 2 and 4 using component waivers. The last two columns use state stratified hazards referred for convenience below as the "state stratified" model. The coefficients in the tables are exponentiated so that a value less than one is a reduction in the hazard and a value greater than one is an increase in the hazard. Exponentiated coefficients help to interpret the size of the effects. For example, the coefficient on being black in the first model of .593 indicates that the underlying hazard for blacks is only about two-thirds as high as that for non-blacks.

Among demographic variables, being black has a significant negative effect on exits from headship as expected in all models. In the model with state stratified hazards, AFDC benefits have a significant positive effect on exiting headship. Given the fixed effects in the model, this

[7] The underlying hazard can be recovered as a step function with one step for each completed spell length in the sample (with a separate underlying hazard for each state in the stratified version).

[8] Chamberlain (1985) and Kalbfleish and Prentice (1980) suggest the potential for a partial likelihood approach to eliminate fixed effects. The model is applied to control for family specific effects in Ridder and Tunali (1999) and to control for location specific effects in a model of welfare duration by Fitzgerald (2000).

says that states with rising benefits saw higher rates of exit, a counterintuitive result that may reflect other attributes of the states welfare systems. Fitzgerald (2000) also observes this counterintuitive result in models of welfare durations. The local employment conditions variables do not have significant effects.

The work-type waiver aggregate has a significant positive effect in the state stratified model. States that adopt work-type waivers have higher exit rates from headship. The term/family aggregate waiver does not have a significant effect on exits. Model 4 (state stratified) allows us to see which component waivers may be driving the result. Both the earnings disregard and the JOBS waiver indicators have large positive coefficients. The positive effect on exits of the JOBS waiver is consistent with the conceptual model that waivers that make welfare receipt more onerous should encourage exit from headship. But the sign on the earnings disregard is not consistent with that statement since a higher disregard raises income while on welfare. Perhaps all that can be said is that waivers that encourage work tend to increase exits. We also see a significant negative effect of the family cap waiver on exits. Since family caps reduce welfare generosity, one would expect that caps would discourage headship and increase exits. Our empirical result is counterintuitive.

In specification tests, we added the additional political variables and fraction of never-married heads. The results were robust. When we used the any waiver indicator, its coefficient was significantly non-zero apparently due to the work-type waivers. When we used the waiver dates lagged nine months, the results were very similar except that the teenage coresidence indicator had a marginally significant (10 percent) positive coefficient. The use of a nine month lag reduces the number post-waiver observations which may make the results less robust. The teenage coresidence requirement would have a positive impact on exits from headship if it encouraged marriage or encouraged children to move out. Neither seems very likely from a theoretical perspective. But the results may indicate that more time will be needed to assess the impact of the teenage coresidence waiver.

The mixed results and the large size of some of the waiver coefficients make us somewhat skeptical that the results can be taken at face value. We note as well that we may have a small sample problem: some of the waiver indicators will have a relatively small number of exits in each state after the waiver takes effect.[9] Nevertheless, we do observe that work-encouraging waivers appear to speed exits from headship.

2. Entry Rates into Female Headship

A woman enters female headship in one of two ways: an unmarried women has a pre-marital birth (or children move in) or a married women with children becomes divorced. We estimate general headship entry hazard models which combine both of these paths. The estimation results are shown in Table 6.

[9] The Census Bureau requested that we suppress the coefficient on the Term Limit due to concerns that its value might reveal confidential information. Few exits occurred in the sample in the adopting states after the adoption of this waiver.

In the models, the only significant demographic variable is being black, and this increases the entry rate into headship. The local labor market variables both have significant effects. A woman who faces a higher real wage in her area has a higher entry rate into headship. As noted earlier, wages have competing effects: higher wages raise the opportunity cost of children, which should reduce births and entry into headship, but higher wages have ambiguous effects on marriage. The independence effect of higher wages could encourage divorce, hence entry. The predicted employment probability reduces entry rates. Women in areas with lots of employment at their skill level are less likely to become female heads. This could reflect lower fertility by women with better job prospects. The AFDC benefit has a positive effect on entry in the unstratified models, as predicted by theory. But when we stratify by state (allowing each state to have its own underlying hazard), the effect becomes insignificant. This again suggests that benefits may be picking up other unmeasured attributes of the states in the unstratified model. The maximum EITC benefit behaves similarly. An increase in the EITC reduces the entry rate to headship, but the effect becomes statistically insignificant once we stratify by states.

The work-type aggregate waiver shows a significant effect in reducing entry rates. The term/family type waiver does not show a statistically significant effect. Again, the work encouraging waivers appear to have a beneficial effect in reducing entry rates. When we turn to the component waivers, all are statistically insignificant except for earnings disregards in the unstratified model. Higher earnings disregards are estimated to reduce entry rates, again a counterintuitive result. We again are left with a pattern of results that makes us wary but nevertheless suggest that work-type waivers reduce headship.

For specification checks, we reestimated the entry and exit models including the political variables and fraction of never-married female heads and the results were robust.[10] We also attempted to estimate the models with a low-education/waivers interaction. Unfortunately, the sample size of completed spells by low education women is too small to be precise. In a specification with the aggregate waiver variable, the coefficient is not statistically significant, and the remaining coefficients are robust. When we used the nine month lag, neither the aggregate waivers nor the component waivers were significant at the 10 percent level. This further reduces our confidence that waivers have an impact.

D. Transition Rates from the Pooled New Spell and Retrospective Data

1. Spell Construction

Among its topical modules, the SIPP collects retrospective information on marriage, fertility, and migration. The topical module that contains this information occurs in the second interview of the survey. The retrospective data on marriage includes marriage beginning and ending dates for up to three marriages. We exclude the very small number of women with four or more marriages. The fertility module includes birth dates for the first and last child of the mother. Together these modules can be used to infer periods of female headship under some strong assumptions. We use the information to backcast periods prior to the sample that the

[10] An earlier version of this paper used monthly data. The coefficients on covariates in both entry and exit hazards were similar in sign to those in this paper, but the z-statistics and significance levels vary somewhat. The previous paper did not use the two aggregate waivers work-type and term/family type.

woman was unmarried and had her own child living with her, i.e. periods when the woman was a female head. We backcast headship to age 15 for all sample women. We do this by defining a female headship indicator for each wave (4 month period) going back from the woman's age at the beginning of the panel to age 15. We then construct spells of headship and non-headship based on the sequence of indicators. Since our contextual data on wages and employment probabilities only goes back to 1971, we restrict our sample to women who are 15 or less in 1971. This narrows our sample during the period of the SIPP to women aged 15 to approximately 35.

This method assumes that the retrospective information and the core information can be mixed. I refer to the current data from the core SIPP survey instrument as the core data. The retrospective information has a different recall structure than the four month recall used in the core data. The quality of the retrospective information may be more questionable due to its longer recall, but, for our purposes, we are using only dates of personal significance that may be well recalled: marriage dates, children birth dates, and, below, residence information.

Our construction of spells requires further strong assumptions. We assume that the woman has a child living with her from the birth date of her first child until the date in which the last child turns 18. This will be false for women whose children die or move out to live with others. We know that this happens in a non-trivial number of cases.

This gives rise to a type of seam problem: the retrospective data and core data can disagree. Later we refer to this as the retro-core seam. In particular, the retrospective data may indicate that a woman is unmarried and has a child at month 1 of the panel whereas the core data shows an unmarried woman without a child. This occurs because of our assumption for the retrospective data that all children ever born are alive and living with the mother when in fact the child may have moved out or died. In this situation, we assume that the current core data is correct and infer that the child has moved out or died prior to wave 1. This results in two types of false spell endings and beginnings. One is the headship spell that appears to begin in the retrospective period and then appears to terminate at the beginning of the core data. The second is the spell of non-headship that appears to begin at the beginning of the core data.[11] We would like to refine the spell dating so that we can determine the true beginning dates and end dates of spells of these types. But SIPP does not contain enough retrospective information to determine when children moved out of their households. Lacking that, we drop these spells. Note that it would not be correct to treat these spells as censored since we lack information on the spell beginning as well as the end. As a specification check we also run results using the spells and treating the ending and beginning dates as legitimate. We recognize that these second results are clearly using badly dated spells.

To establish contextual information on labor markets and welfare policy for the retrospective spells, we need to determine the state and county residence during the retrospective period. We do this using the retrospective migration data from SIPP. Unfortunately, the SIPP retrospective migration data is not ideal for our purposes. It asks about length of time in current

[11] The error in the spells at the seam may, of course, indicate that earlier spells constructed from the retrospective information are in error. For example if a child dies as an infant we could potentially generate a series of errant spells.

residence (county and state) and asks about previous state residences going back for two moves prior to the current residence. Since we want to use county labor market information, we can only infer county of residence back in time for those who have not moved among counties. We thus restrict our sample to those spells that have occurred while the woman lived in the same county as the initial county of residence in the SIPP core data. This obviously causes a sample selection bias if these non-movers' spells differ systematically from the population of spells of female heads or female non-heads. One might expect, for example, that the non-movers will tend to have longer spells of non-headship since we exclude spells that end with an extra-county move which may have been due to divorce. Similarly, we exclude spells of headship that end with an extra-county move due to a marriage.

2. Exit Rates from Female Headship: Pooled New and Retrospective Spells

Table 7 shows results from the pooled sample of new and retrospective spells, with the retro-core-seam spells deleted, hereafter called the pooled sample. The coefficients on the personal variables are consistent between the pooled and new sample. Being black has a significant impact and reduces exits; age significantly reduces exits in the pooled sample. The employment probability has a significant effect in encouraging exits in the pooled sample, but was insignificant in the new spells sample. Perhaps the longer time series of the pooled sample adds variation and thus precision.

As for waivers, neither of the aggregate waivers are significant with the pooled sample. Among component waivers, the JOBS waivers have significant positive impacts in the state-stratified model, which is the same result obtained with the new spell sample. The earnings disregard waiver was significant and positive in the new spells sample, but is insignificant in the pooled sample. The use of a nine month lag produces a positive and significant coefficient on work-type waivers that is largely due to the earnings disregard component. This result differs from the unlagged retrospective result, but is consistent with the new spell results. We also ran hazards using the any waiver indicator and find that it is not significantly non-zero (five percent). Overall, we conclude that the pooled sample gives similar results to the new spell sample for the exit hazard.[12]

2. Entry Rates from Female Headship: Pooled New and Retrospective Spells

The entry rate hazards from the pooled sample are shown in Table 8. The personal and labor market coefficients are similar to those of the new spells sample in Table 6. An exception is that the predicted log wage does not have a significant positive impact as it did in the new spells sample. The aggregate waivers are not significant in the pooled sample. The term limit waiver has a significant impact on entry, but the impact is positive—it encourages entry, again a counterintuitive result. The use of the any waiver indicator or the use of nine month lags produced no significant waiver effects.

[12] We also estimated results from a second pooled sample wherein we did not delete the retro-core seam spells. The results differ somewhat and look more like the results from the new spells. This perhaps also attests to the robustness of the new spell findings, but the fact that this second pooled sample has known errors cause us to put little reliance on the results.

Although we recognize the limitations of our retrospective sample, the similarity in the models from the pooled and new spell sample give us confidence that the results from the new spell sample are not overturned when we account for existing spells. The waiver results from the new spell sample are slightly stronger. This might suggest that those just beginning spells of headship and non-headship in the 1990s respond more to the waivers than those in earlier spells. But the waiver impacts are not strong enough or stable enough to push the claim.

VI. Conclusion

We began by noting that states that adopted waivers did not see as large a rise in female headship as states that did not adopt waivers. This suggested that waivers have had a beneficial effect in reducing female headship. The result was confirmed by a simple regression. Yet in a multivariate model of levels of headship, waivers were not significant predictors. In transition models for exit from and entry into female headship based on new spells, work type waivers (aggregating the work requirement time limits, JOBS sanctions, and earnings disregard waivers) that encourage or require work were shown to increase exit rates and decrease entry rates. If a goal is to reduce the incidence of female headed families, these are beneficial effects. But in transition models based on combined new spell and retrospective spell sample and in other robustness checks, we do not observe significant work-type waiver effects. Even if we accept the results from the cleaner new spells sample, the mechanism by which work–type waivers reduce headship is not completely clear. Conceptually waivers that encourage work have ambiguous effects on fertility and marriage. When we turn to the individual component waivers, they are sometimes significant but are also more difficult to interpret. For example, earnings disregard waivers that allow women to keep more of their earnings while on welfare appear to speed exits from headship and reduce entry. This is not what one would expect from a waiver that makes welfare more attractive. But expanded disregards also raise wages and earnings while on welfare which could affect marriage options. So we conclude that work-type waivers might have an impact, but the evidence is far from clear. In addition, we find that local employment conditions and wages are important determinants of transitions out of female headship.

Two potential problems could produce weak effects among the component waivers. One is simply the colinearity among the component waivers. States adopt bundles of waivers that limit the variation in adoption dates among the components. Second, our sample might not include enough time after the adoption of waivers. Especially in models with fixed state effects or stratified by state, we put great demands on the time series variation within each state to sort out effects. As mentioned earlier, it may take time for people to respond to waiver provisions or some might respond in anticipation of waiver requirements. An obvious extension of our work would be to add in later data such as the 1996 panel of the SIPP, that would include more time after the adoption of waivers. The conceptual difficulty that must be overcome to do so is how to handle the many changes from “full blown” welfare reform that occur in 1996.

In the future, since the welfare decision and female headship decision interact, we ultimately plan to estimate a joint hazard model of the female headship decision and the welfare recipiency decision using the techniques of Lillard (1993) for simultaneous hazards.

In short, based on data through 1995, the paper produces weak evidence that work-encouraging waivers had a beneficial effect by reducing female headship of families. But the mechanism by which work-encouraging waivers affect headship are not clear and the finding is not robust. We find little evidence that family caps, teenage coresidence requirements or termination limits will reduce the number of single-parent families.

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Appendix: Development of the Likelihood Function for Partial Likelihood Estimation of the Cox Proportional Hazard Model

Let t_{ij} denote the (uncensored) spell length by the *i*th woman in location *j*. Define the hazard as

(1) $$\Psi(t_{ij} \mid X_{ij}(t_{ij})) = h(X_{ij}(t_{ij}))\, \lambda_j(t_{ij})$$

where

(2) $h(X_{ij}(t)) = \exp(B'X_{ij}(t_{ij}))$ and X denotes the matrix of potentially time varying covariates; B is a vector of unknown coefficients; $\lambda_j(t_{ij})$ is the baseline hazard in location j.

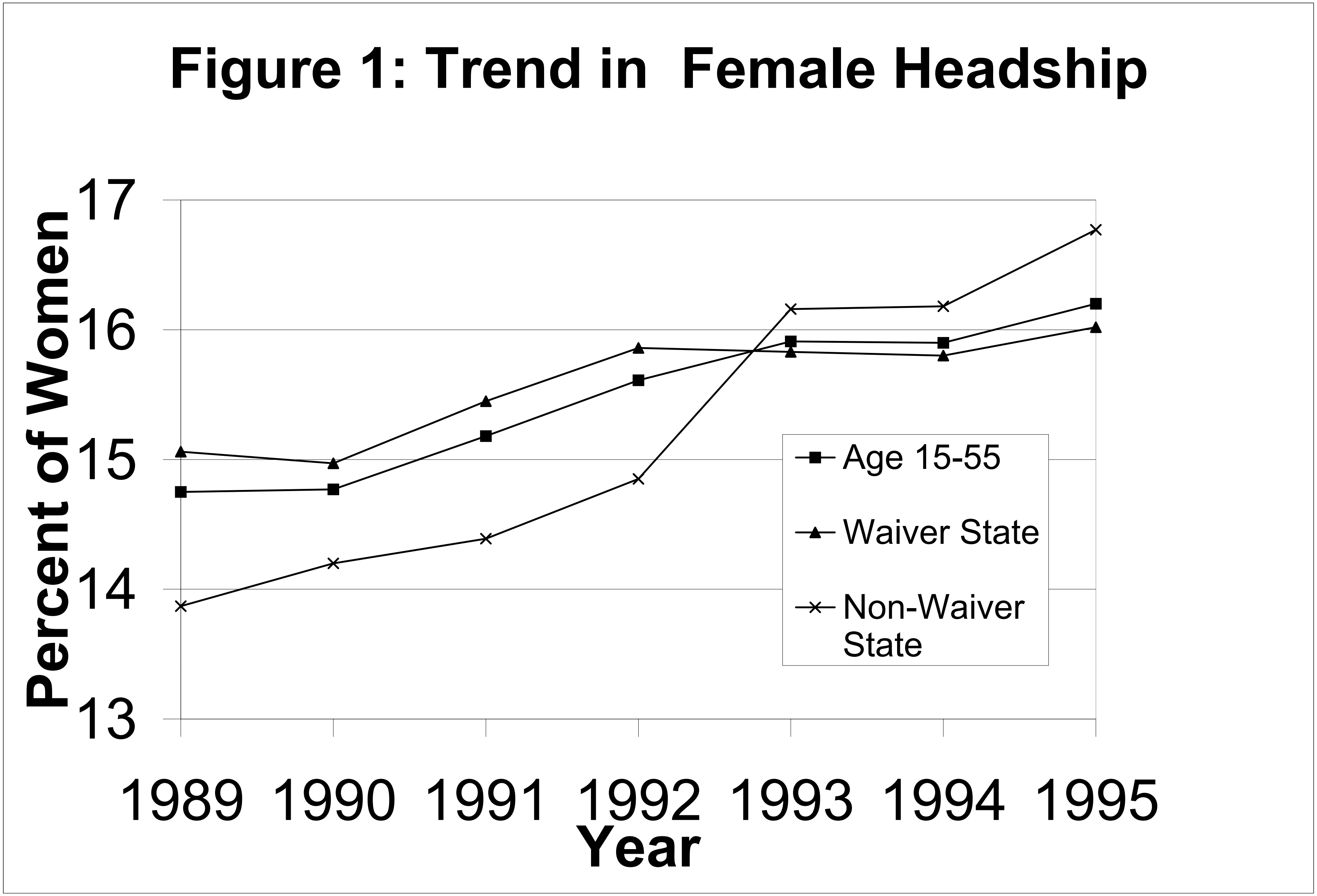
(3) $$L_i(B) = \frac{\Psi(t_{ij} \mid X_{ij}(t_{ij}))}{\sum_{k \varepsilon R_{ij}} \Psi(t_{ij} \mid X_{kj}(t_{ij}))}$$

(4) $$= \frac{h(X_{ij}(t_{ij}))\lambda_j(t_{ij})}{\sum_{k \varepsilon R_{ij}} h(X_{kj}(t_{ij}))\lambda_j(t_{ij})}$$

(5) $$= \frac{\exp\left(B'X_{ij}(t_{ij})\right)}{\sum_{k \varepsilon R_{ij}} \exp\left(B'X_{kj}(t_{ij})\right)}.$$

Everything is measured at t_{ij}, the spell length for person *i*. Note that the underlying hazard λ_j in (4) has canceled out. In essence, the risk set for a person includes only those in her location, and the estimation makes comparisons only among those who live in location *j*. The likelihood for the whole sample is

(6) $$L = \prod_{i=1}^{N} L_i(B) \quad \text{where } L_i(B) \text{ is from (3).}$$



Source: 1990,1992,1993 Panels of SIPP. Sample of Women Aged 15-55.

Table 1
Sample Means for Variables

A. For Women age 15-55, All person waves
(Sample used in Female Headship Levels Logits, Unweighted)

Variable	Mean
Proportion of Female Heads	0.154
Education: Highest Grade Completed	13.1
Person Lives in an MSA	.79
Age	36.7
Black	.118
AFDC benefits for family of 3 ($1997)	460
Maximum EITC for family with 2 children	1839
Predicted Local Log Real Wage	1.65
Predicted Local Probability of Employment	0.73
Proportion of States that Ever Adopted the Indicated Waiver through 1992-1996 (Not from SIPP data)	
Any major waiver	0.75
Term Limits	0.28
Work Requirement time limit	0.23
Family Cap	0.49
Jobs sanctions	0.47
Enhanced earnings disregard	0.44
Teenage Mother Coresidence Required	0.49
Sample size (person waves)	300540

B. For Women Age 15-55, Means averaged over spells (Unweighted)

	Spells of Non-headship (for entry rate hazard)	Spells of Headship (for exit rate hazards)
Age	33.9	33.9
Black	0.18	.15
Education: Highest Grade Completed	12.4	12.4
Predicted Probability of Employment	0.72	0.72
Predicted Log Real Wage	1.60	1.59
AFDC benefits for family of 3 ($1997)	451.	448.
Maximum EITC for family with 2 children	1807.	1756.
Sample size (person months)	10550	11170

Table 2
Trends in Female Headship
(Dependent Variable =1 for female heads, 0 for non-heads)

	Model (1)	Model (2)	Model (3)	Model (4)
Year	0.006*	0.003*	0.002	0.002+
	(2.45)	(2.04)	(0.69)	(1.70)
Ever any waiver	8.313			
	(1.51)			
Year* ever any waiver	-0.004			
	(1.51)			
Any waiver (year by year)		13.049*		
		(2.28)		
Year*any waiver		-0.007*		
		(2.28)		
Ever term/family waiver			-11.626	
			(1.50)	
Ever work type waiver			11.733+	
			(1.85)	
Year*ever term/fam waiver			0.006	
			(1.50)	
Year*ever work type waiver			-0.006+	
			(1.86)	
Term/Family waiver (by year)				-11.062
				(1.31)
Work type waiver (by year)				17.535**
				(2.73)
Year*term/fam waiver (by year)				0.006
				(1.31)
Year* work type waiver (by year)				-0.009**
				(2.73)
Constant	-11.490*	-5.430*	-3.825	-4.534+
	(2.42)	(1.98)	(0.66)	(1.65)

Table 2 (continued)	Model (1)	Model (2)	Model (3)	Model (4)
Observations	256132	256132	256132	256132
Root MSE	.36323	.36324	.26321	.36323
F stat for test of equality of term/fam and work type waiver coeffs			15.1**	8.05**

Sample of women aged 15-55 from 1990,92,93 panels of SIPP. Robust t-statistics in parentheses (adjusted for clustering by person).
+ significant at 10%; * significant at 5%; ** significant at 1%

Source: wave/logit 4 dw-waiver

Table 3
Logit Regressions for Female Headship

	Model (1)	Model (2)	Model (3)	Model (4)
Term/Family Waiver	0.032		0.024	
	(0.70)		(0.51)	
Work Type Waiver	0.0002		-0.037	
	(0.004)		(0.84)	
Term Limit		-0.045		-0.023
		(0.44)		(0.22)
Work Req. Time Limit		0.051		-0.054
		(0.68)		(0.71)
Family Cap		-0.003		-0.010
		(0.05)		(0.15)
JOBS waiver		0.075		-0.004
		(1.13)		(0.06)
Earnings Disregard		-0.065		-0.061
		(1.41)		(1.31)
Teen Coresidence Required		0.050		0.099+
		(0.97)		(1.88)
Age	-0.055**	-0.056**	-0.056**	-0.056**
	(33.99)	(34.04)	(33.59)	(33.59)
Black	1.573**	1.573**	1.607**	1.607**
	(44.75)	(44.73)	(43.72)	(43.72)
MSA resident	-0.001	0.001	-0.008	-0.008
	(0.01)	(0.03)	(0.18)	(0.18)
Education	-0.127**	-0.127**	-0.132**	-0.132**
	(16.71)	(16.69)	(16.73)	(16.73)
Predicted Employment Probability	-0.808**	-0.830**	-0.757**	-0.758**
	(4.62)	(4.74)	(4.08)	(4.09)
Predicted Log real wage	0.110	0.117	0.180+	0.181+
	(1.33)	(1.40)	(1.95)	(1.96)
AFDC benefits/100	0.0449**	0.0470**	-.0282	-.0439
	(5.12)	(5.32)	(0.50)	(0.78)

Table 3 (continued)	Model (1)	Model (2)	Model (3)	Model (4)
Max EITC/100	-.0130	-.0150	-.0047	-.0103
	(1.43)	(1.65)	(0.28)	(0.62)
Year Effects	Yes	Yes	Yes	Yes
State Effects	No	No	Yes	Yes
Log Likelihood	-114636.3	-114626.4	-114270.2	-114264.0
Observations	300540	300540	300540	300540

Notes: Sample of Women aged 15-55 from 1990,92,93 Panels of SIPP. Robust z-statistics in parentheses (adjusted for clustering by person).
+ significant at 10%; * significant at 5%; ** significant at 1%
Source wave/logit 4 dw-waiver

Table 4 Logit for Female Headship
With Low Education Interaction

	Model (1)	Model (2)	Model (3) State Stratified	Model(4) State Stratified
Term/Family Waiver	-0.025 (0.49)		-0.035 (0.66)	
Work-type Waiver	-0.003 (0.06)		-0.045 (0.94)	
Low Ed * Term/family Waiver	0.311** (2.88)		0.327** (3.00)	
Low Ed * Work Type Waiver	0.031 (0.34)		0.050 (0.56)	
Term Limit		.0059 (0.05)		0.030 (0.26)
Work Req. Time Limit		0.088 (1.02)		-0.016 (0.18)
Family Cap		.008 (0.11)		-0.004 (0.056)
JOBS waiver		-0.019 (0.25)		-0.094 (1.28)
Earnings Disregard		-0.070 (1.35)		-0.073 (1.40)
Teenage Coresidence Required		0.011 (0.188)		0.056 (.974)
Low Ed * Term Limit		-0.306 (1.18)		-0.304 (1.19)
Low Ed * Work Req. Time Limit		-0.167 (0.92)		-0.155 (0.84)
Low Ed* Family Cap		-.030 (0.181)		-0.008 (0.050)
Low Ed * JOBS		0.525** (3.16)		0.516** (3.13)
Low Ed * Earnings Dis.		0.039 (0.39)		0.049 (0.49)
Low Ed * Teen Coresidence		.190 (1.40)		.206 1.51
Time Dummies	Yes	Yes	Yes	Yes
Observations	300540	300540	300540	300540
Log Likelihood	-114609.61	-114585.6	-114239	-114220.2

All Models include time dummies and age, black, education, MSA residence, Probability of Employment, Predicted Area Wage Rate, real AFDC benefits for family of 3, and Maximum EITC amount. Robust z-statistics in parentheses.

+ significant at 10%; * significant at 5%; ** significant at 1%

Table 5
Hazard for Exit from Female Headship
(Cox Proportional Hazard by Partial likelihood)

	Model (1)	Model (2)	Model (3) State Stratified	Model (4) State Stratified
Term/Family waiver	1.113		1.120	
	(0.54)		(0.39)	
Work type waiver	1.257		1.971*	
	(1.29)		(2.30)	
Term Limit		>1 Insignif		<1 Insignif
Family Cap		0.569+		0.376*
		(1.69)		(1.99)
Teen Coresidence Required		1.442		1.211
		(1.40)		(0.54)
Work Req. Time Limit		0.597		0.864
		(1.33)		(0.26)
JOBS waiver		1.467		2.975*
		(1.44)		(2.39)
Earn Disregard		1.361		2.941**
		(1.57)		(3.03)
Age	0.998	0.998	0.999	0.999
	(0.27)	(0.32)	(0.11)	(0.10)
Black	0.593**	0.596**	0.552**	0.554**
	(2.86)	(2.83)	(3.02)	(3.00)
Education	1.031	1.032	1.041	1.041
	(0.94)	(0.98)	(1.17)	(1.16)
MSA resident	0.948	0.946	0.984	0.984
	(0.37)	(0.40)	(0.10)	(0.10)
Predicted Employment Prob.	0.609	0.567	0.547	0.546
	(0.72)	(0.82)	(0.82)	(0.82)
Predicted Log Real Wage	0.732	0.753	0.712	0.714
	(0.88)	(0.80)	(0.87)	(0.86)
AFDC benefits	1.000	0.999	1.006*	1.009**
	(1.28)	(1.56)	(1.99)	(2.73)
Max EITC	1.000	1.000	1.001	1.001
	(0.26)	(0.39)	(0.89)	(1.19)

Table 5 (continued)	Model (1)	Model (2)	Model (3)	Model (4)
Time Effects	Yes	Yes	Yes	Yes
Log Likelihood	-2366.7	-2364.7	-1242.34	-1236.35
Chi-Sq for all coeffs =0	109.5**	113.5**	108.1**	120.09**
Observations	10550	10550	10550	10550
Persons	2740	2740	2740	2740
Complete Spell	321	321	321	321

Notes: Sample of first observed spell of headship for women aged 15-55 from 1990,92,93 panels of SIPP. Absolute value of z-statistics in parentheses. Coefficient on Term limits suppressed for confidentiality.
+ significant at 10%; * significant at 5%; ** significant at 1%

Table 6
Hazards for Entry to Female Headship
(Cox Proportional Hazard by Partial Likelihood)

	Model (1)	Model (2)	Model (3) State Stratified	Model (4) State Stratified
Term/Family waiver	1.480		1.554	
	(1.60)		(1.20)	
Work Type waiver	0.578*		0.352*	
	(2.47)		(2.57)	
Term Limit		>1 insignif		>1 Insignif
Family Cap		1.640		1.158
		(1.28)		(0.27)
Teen Coresidence Required		1.168		1.589
		(0.49)		(1.09)
Work Req. time Limit		1.456		1.168
		(0.82)		(0.26)
JOBS waiver		0.675		0.580
		(1.04)		(0.97)
Earnings Disregard		0.668+		0.528
		(1.66)		(1.44)
Age	0.994	0.994	0.996	0.995
	(0.68)	(0.70)	(0.44)	(0.51)
Black	2.073**	2.061**	2.059**	2.068**
	(4.65)	(4.59)	(4.33)	(4.35)
Education	0.985	0.988	0.978	0.982
	(0.39)	(0.32)	(0.54)	(0.45)
MSA resident	0.932	0.943	0.862	0.872
	(0.39)	(0.32)	(0.74)	(0.69)
Predicted Employment Prob.	0.196*	0.195*	0.231+	0.230+
	(1.97)	(1.97)	(1.68)	(1.69)
Predicted Log real wage	2.156+	2.173+	2.354+	2.290+
	(1.80)	(1.82)	(1.84)	(1.78)
AFDC Benefits	1.001*	1.001*	0.997	1.000
	(2.46)	(2.33)	(0.77)	(0.09)
Max EITC	0.998**	0.998**	0.998	0.998
	(2.72)	(2.60)	(1.09)	(0.92)

Table 6 (continued)	Model (1)	Model (2)	Model (3)	Model (4)
Time Effects	Yes	Yes	Yes	Yes
Log Likelihood	-1658.6	-1658.9	-896.38	-896.47
Chi-Sq for all coeffs =0	107.9**	107.3**	95.15**	94.9**
Observations	11170	11170	11170	11170
Persons	2755	2755	2755	2755
Complete spell	225	225	225	225

Notes: Sample of first observed spell of non-headship by Women aged 15-55 from 1990,92,93 Panels of SIPP. Absolute value of z-statistics in parentheses. Coefficient on Term suppressed for confidentiality.
+ significant at 10%; * significant at 5%; ** significant at 1%

Source: cox3 dw-waiver

Table 7
Exit Hazards for Pooled Retro/Core Sample

	Model (1)	Model (2)	Model (3) State Stratified	Model (4) State Stratified
Term/Family waiver	0.952 (0.33)		1.053 (0.27)	
Work type waiver	1.157 (1.20)		1.293 (1.58)	
Term Limit		0.930 (0.17)		0.882 (0.24)
Family Cap		1.019 (0.08)		0.982 (0.06)
Teen Coresidence Required		0.910 (0.43)		0.843 (0.66)
Work Req. Time Limit		0.867 (0.46)		0.839 (0.45)
JOBS waiver		1.231 (1.02)		2.015* (2.54)
Earn Disregard		1.095 (0.67)		1.146 (0.78)
Age	0.992 (0.99)	0.993 (0.94)	0.985+ (1.79)	0.985+ (1.80)
Black	0.464** (9.29)	0.464** (9.28)	0.441** (9.04)	0.443** (8.98)
Education	0.984 (0.74)	0.985 (0.70)	0.982 (0.80)	0.982 (0.79)
MSA resident	0.929 (0.90)	0.929 (0.91)	0.906 (1.06)	0.906 (1.07)
Predicted Employment Prob.	2.517* (2.58)	2.512* (2.57)	2.118+ (1.94)	2.146* (1.98)
Predicted Log Real Wage	0.976 (0.12)	0.958 (0.22)	1.217 (0.83)	1.211 (0.81)
AFDC benefits	1.000+ (1.80)	1.000 (1.64)	1.002* (2.04)	1.002 (1.59)
Max EITC	1.000 (0.39)	1.000 (0.41)	1.000 (1.03)	1.000 (1.05)
Time Dummies	Yes	Yes	Yes	Yes
State Stratified	No	No	Yes	Yes
Observations	33829	33829	33829	33829
Spells	3835	3835	3835	3835
Complete Spell	1127	1127	1127	1127
Log Likelihood	-8104.3	-8103.6	-4248.4	-4244.8
Chi Square for zero coeff.	254.8	256.3	241.7	249.0

Notes: Sample of Persons who did not move across counties. Seam spells deleted.
Absolute value of z-statistics in parentheses
+ significant at 10%; * significant at 5%; ** significant at 1%

Table 8
Entry Hazard for Pooled Retro/Core Sample

	Model (1)	Model (2)	Model (3) State Stratified	Model (4) State Stratified
Term/Family waiver	1.158 (0.93)		1.361 (1.58)	
Work type waiver	1.095 (0.70)		1.085 (0.50)	
Term Limit		2.635** (2.86)		2.389* (2.01)
Family Cap		1.309 (1.13)		1.618 (1.62)
Teen Coresidence Required		0.971 (0.12)		0.825 (0.67)
Work Req. Time Limit		0.878 (0.37)		1.124 (0.29)
JOBS waiver		1.305 (1.18)		1.511 (1.46)
Earn Disregard		0.888 (0.83)		0.790 (1.34)
Age	1.042** (5.91)	1.043** (6.09)	1.046** (5.85)	1.046** (5.92)
Black	2.641** (14.60)	2.650** (14.63)	2.731** (13.92)	2.734** (13.93)
Education	0.919** (4.82)	0.921** (4.64)	0.908** (5.07)	0.908** (5.07)
MSA resident	0.930 (0.90)	0.945 (0.70)	0.881 (1.40)	0.885 (1.34)
Predicted Employment Prob.	0.579+ (1.76)	0.552+ (1.91)	0.560+ (1.75)	0.567+ (1.71)
Predicted Log Real Wage	0.844 (0.89)	0.818 (1.05)	0.905 (0.44)	0.884 (0.55)
AFDC benefits	1.000 (0.82)	1.000 (0.39)	1.001 (0.70)	1.000 (0.21)
Max EITC	1.000 (0.09)	1.000 (0.12)	1.000 (0.77)	1.000 (0.95)
Time Dummies	Yes	Yes	Yes	Yes
State Stratified	No	No	Yes	Yes
Observations	100966	100966	100966	100966
Spells	8629	8629	8629	8629
Complete Spell	1200	1200	1200	1200
Log Likelihood	-9250.8	-9243.0	-5172.5	-5165.2
Chi Square for zero coeff.	629.1**	644.7**	594.4**	608.9**

Notes: Sample of Persons who did not move across counties. Seam spells deleted.
Absolute value of z-statistics in parentheses
+ significant at 10%; * significant at 5%; ** significant at 1%